unknown in higher latitudes. This blue patch does not apparently always coincide exactly with the barometric centre. The author's researches show that in middle latitudes the formation of a bull's-eye does not take place when the motion of translation is rapid ; but as this blue space is not observed in British cyclones when they are moving slowly, it would appear that a certain intensity of rotation is necessary to develop this phenomenon.

The trough phenomena—such as a squall, a sudden shift of wind and change of cloud character and temperature just as the barometer turns to rise, even far from the centre—which are such a prominent feature in British cyclones, have not been even noticed by many meteorologists in the tropics. The author, however, shows that there are slight indications of these phenomena everywhere ; and he has collated their existence and intensity with the velocity of propagation of the whole mass of the cyclone.

Every cyclone has a double symmetry. One set of phenomena such as the oval shape, the general rotation of the wind, the cloud ring, rain area, and central blue space, are more or less related to a central point. Another set, such as temperature, humidity, the general character of the clouds, certain shifts of wind, and a particular line of squalls, are more or less related to the front and rear of the line of the trough of a cyclone.

The author's researches show that the first set are strongly marked in the tropics, where the circulating energy of the air is great and the velocity of propagation small ; while the second set are most prominent in extra-tropical cyclones, where the rotational energy is moderate and the translational velocity great.

The first set of characteristics may conveniently be classed together as the rotational ; the second set as the translational phenomena of a cyclone.

Tropical and extra-tropical cyclones are identical in general character, but differ in certain details due to latitude, surrounding pressure, and to the relative intensity of rotation or translation.

" Conduction of Heat in Liquids." By C. CHREE, B.A., King's College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received March 31,—Read April 21, 1887.

The conduction of heat in liquids has of late years been considered by several observers in Germany. In this country Mr. J. T. Bottom-ley and Professor Guthrie carried out experiments a good many years ago, but in neither case do the results agree well with those obtained abroad. In all the more recent methods the conduction has taken

place through thin layers of the liquid, and thus in interpreting the
results, the conditions at the surfaces limiting the liquid layer are of
primary importance. It has been assumed by each observer that con-
tiguous surfaces of any two media are in all circumstances at the same
temperature. This, however, is contradicted by some high authorities,
so it would seem important to have independent results based on
experiments in which the liquid layer is of considerable thickness.

It should also be noted that in methods employing thin layers the
temperature varies so rapidly in passing from one surface to the other
that the liquid forms a by no means very homogeneous medium. This
is the more important because experiments indicate that the conduc-
tivity of most if not all liquids increases rapidly as the temperature
rises.

The following experiments were carried out in the Cavendish
Laboratory at the suggestion of Professor J. J. Thomson, to whom I
am much indebted for suggestions as to the form of the apparatus
and the methods to be employed.

Two series of experiments were made with different apparatus. In
the earlier series it was found that the apparatus was too large to be
conveniently worked, and few results of a satisfactory nature were
obtained. In the second series the apparatus was much reduced in
size, though otherwise closely resembling that first employed. It will
thus be sufficient to describe the second form and supply data as to the
size of the first.

The liquid was contained in a wooden tub with vertical sides,
19·15 cm. in diameter, which was carefully fitted up by the mechanic
at the Cavendish Laboratory. Not far below the rim and at equal
distances apart were fixed three conical wooden pegs. The axes of
the pegs formed parts of radii of a horizontal section of the tub, pro-
jecting inwards from the cylindrical surface to a distance somewhat
exceeding 2 cm. The pegs supported a flat, thin-bottomed dish of
tin-plate, 14·85 cm. in diameter, whose base was thus maintained
horizontal. The liquid was poured into the tub till it reached the
base of the dish. The liquid surface being strictly horizontal, it was
easy to judge by the eye whether the dish was so also; if not the tub
had to be adjusted till it was so. It was then advisable to stir the
liquid to make sure that no air bubbles remained clinging to the dish.
The bottom of the dish was about 5·2 cm. above that of the tub.

The method required the temperature to be measured at a known
depth below the liquid surface. This end was secured by measuring
the electrical resistance of a fine straight platinum wire 6·6 cm. long,
which was supported at a depth of 2·61 cm. below the surface by two
small trestles of glass. These were fixed in the bottom of the tub and
projected upwards. The platinum wire was drawn tight over them,
each end being fused to a much thicker piece of copper wire, so that

the junctions were exactly at the same depth and immediately below the level of the platinum wire. The copper wires were tied with silk to the vertical parts of the trestles, and, passing straight down, were led through the bottom of the tub. The middle point of the platinum wire was vertically below the centre of the dish, and thus its ends were much nearer the axis of the tub than was the rim of the dish. The strictly horizontal position of the wire was tested by pouring in water first to the level of the wire and then to that of the dish. It was thus made certain that when the tub was placed so that the dish was horizontal, the platinum wire was so also. For most liquids the pegs, wires, &c., were secured by sulphur, but for bisulphide of carbon this was replaced by asbestos. During the experiments the tub was placed inside a double-walled wooden box, the space between the walls being stuffed with packing. The box was provided with a double lid similarly stuffed. To the ends of the copper wires were attached binding screws, the wires leading from which passed through grooves cut in the rim of the box. Thus the wires were in no way disturbed in moving the lid. This was a point of some importance, as even the small variations in the electrical resistance produced by slightly disturbing the binding screws was apt to affect the accuracy of the observations.

The platinum and its connecting wires formed one of the resistances of a Wheatstone's bridge arrangement. One of the others was a fixed resistance, and the remaining two were supplied by a wire bridge with a sliding-piece. The resistance of the platinum wire varies with the temperature, and—at least for small variations—its change is proportional to the change of temperature. The current was supplied by a single element—a small Daniell or Leclanché. A galvanometer, whose resistance could be reduced to 0.12 of an ohm, measured the variation from a balance between the resistances. The usual precautions in dealing with small resistances had to be taken; in particular it was found difficult to avoid producing thermoelectric currents if the sliding-piece were moved.

In the first apparatus the tub was 38.2 cm. in diameter, and the dish 30.2. The platinum was coiled in a spiral round a fine horizontal glass tube at a mean depth of 6.45 cm. below the liquid surface and about 7 cm. above the bottom of the tub. The length of the spiral was less than the radius of the tub.

The method of conducting the experiment was as follows:—The tub was put inside the box and filled with the liquid to the level of the dish. The box having been adjusted till the dish was horizontal, the lid was put on. The sliding-piece of the bridge wire was then moved till no current traversed the galvanometer. As the tub and liquid were in general at slightly different temperatures to begin with, some time elapsed before the galvanometer reading became constant.

When this had occurred the sliding-piece was again moved till there was no current through the galvanometer. It was then unnecessary to move the sliding-piece again, unless the deflection became greater than was usual in the experiment. When the galvanometer reading had remained some time constant, the lid of the box was removed and some hot water rapidly poured into the dish, care being taken that none splashed over into the tub. Sometimes the lid was immediately replaced and left on during the whole of the experiment; on other occasions the water was after a certain interval removed by a siphon, ready filled for the purpose. This always left a small quantity of water sufficient to cover the base of the dish without separating into drops.

The battery was connected with a key, and there was another in the galvanometer circuit. In the earlier experiments these were depressed in close succession at intervals of one minute, and the consequent kick or deflection of the needle observed. Subsequently it was found more convenient to use a constant battery, and to keep both keys down during the whole course of the experiment and for some time previously. Both methods were employed for most of the liquids examined, and no difference was detected in the results. The temperature of the platinum wire was seldom raised as much as 2° C. during the experiment, and consequently the disturbance of the balance in the Wheatstone's bridge was small. Thus the current through the galvanometer could be taken as directly proportional to the change in the resistance of the platinum, and so to the rise in its temperature.

Immediately subsequent to the application of the hot water there was a decided increase of the galvanometer reading which ceased very shortly. The reading then remained almost stationary for several minutes. It then began to increase rapidly and continued to rise for a considerable time, though the rate of change began comparatively soon to decrease. At first with the larger tub it was attempted to determine the interval that elapsed before the reading ceased to increase. This was, however, found impracticable, as it required several hours to reach this epoch; and after an hour and a half the rate of change was so slow that the least variation in the temperature of the laboratory was sufficient to upset the experiment. Even with the smaller apparatus this was not a quantity to be conveniently observed. It was found much easier to determine the much shorter interval that elapsed before the platinum wire was being most rapidly heated. This interval could also be expressed conveniently by means of the mathematical theory in terms of the conductivity and other properties of the liquid, and so its determination was sufficient for the purpose in view.

The galvanometer could be made so sensitive that with a single

small cell a deflection exceeding 300 divisions of a millimetre scale could be obtained for a rise of one degree in the tempera- ture of the platinum. When so sensitive as this, however, the galvanometer was too much exposed to the disturbing influences of adjacent currents or the movement of magnets in neighbouring rooms. From eighty to a hundred scale divisions to a degree usually gave the best results, and had the advantage of keeping the spot of light near the centre of the scale during the whole experiment, with- out any movement of the sliding-piece. The sensitiveness was most easily determined by finding how far the sliding-piece on the bridge had to be moved, when the battery was on, to produce a given change in the galvanometer reading. This test was usually applied at the beginning and end of each experiment, as a change in the sensitive- ness during the observations might lead to erroneous conclusions. A slight displacement of the controlling magnet of the galvanometer may occur without affecting to any noticeable extent the position of the zero, and so without some such test as the above a change in the sensitiveness might escape detection.

Theory.

Let v denote the temperature, ρ the density, c the specific heat, and k the conductivity of a given liquid. Suppose the liquid to extend to infinity in every direction, and over the entire plane $x = 0$ a uniform supply of heat to be distributed at a rate given at the time t, counted from the first application of the heat, by the function $f(t)$ per unit area; then at the distance x from the plane of application the temperature at time t is given by

$$2\sqrt{\frac{\pi k}{\rho c}}\, v = \int_0^t \frac{e^{-x^2\rho c/4k(t-\chi)}}{\sqrt{(t-\chi)}} f(\chi)\,d\chi. \quad . \quad . \quad . \quad (1)$$

In the present experiment the base of the dish answers to the plane $x = 0$, and $f(t)$ is to be regarded as proportional to the rate at which heat is conveyed from the dish to the liquid. It is true of course that the liquid exists only on one side of the plane $x = 0$, and does not extend to infinity in any direction. Doubtless the base of the tub tends to reflect the heat that has passed downwards through the liquid, but in the apparatus actually used any such reflected heat would be extremely small, and only the most trifling part of even this effect would show itself within the time of the experiment. Since the length of the platinum wire was much less than the diameter of the dish, which was in turn considerably less than that of the tub, the limitation in the horizontal direction would appear of small conse- quence. In fact when a larger quantity of hot water than was usually employed was poured into the dish, a delicate thermometer indicated

a temperature in the liquid that was sensibly constant at a constant depth except close to the sides of the tub. The absence of liquid on the negative side of the plane $x = 0$ might appear a radical defect. It is clear, however, that in the supposed infinite liquid $\frac{1}{2}f(t)$ will pass into the liquid on each side of this plane, and the existence of the liquid on the one side merely ensures that $\frac{1}{2}f(t)$ is the precise amount passing into the liquid on the other side. But the law of diffusion on either side of the plane can depend only on the heat supplied to that plane, and must be independent of the precise mechanism by which the supply is regulated. For our present purpose it is sufficient to know that $f(t)$ is proportional to the rate at which heat passes into the liquid from the dish, which may be determined by a double observation as follows.

The tub being filled with liquid up to the level of the dish, a certain quantity of water heated to a definite temperature is suddenly poured into the dish. By means of a watch, and a delicate thermometer, raised initially to the temperature of the heated water and with it transferred to the dish, the law of cooling of the water is determined. The quantity of heat lost by the dish per unit of time at any required temperature can be easily deduced. If now the dish be placed on a non-conducting material, and the law of cooling be observed when the other circumstances are the same as before, the quantity of heat which leaves the dish per unit time in the first experiment without passing into the tub is at once obtained for the whole range of temperature. From these two experiments it is not difficult to calculate the amount of heat passing into the liquid in the tub at every instant in that form of the experiment in which the water poured into the dish was left there. When a siphon was employed the capacity for heat of the water left in the dish and the dish itself was so small that the heat subsequently transferred to the liquid was negligible.

To a clear understanding of the use of (1) some knowledge of the expression $t^{-\frac{1}{2}}e^{-x^2\rho c/4kt}$ is desirable. This is proportional to the temperature existing at a depth x in an infinite liquid, originally at zero temperature, at a time t subsequent to the application over the entire plane $x = 0$ of a unit of heat per unit of area. The first and second differential coefficients of the above expression are respectively—

$$t^{-5/2}\, e^{-x^2\rho c/4kt} \left(\frac{x^2\rho c}{4k} - \frac{t}{2}\right),$$

and
$$t^{-9/2}\, e^{-x^2\rho c/4kt} \left\{ \tfrac{3}{4}t^2 - 3\frac{x^2\rho c}{4k}t + \left(\frac{x^2\rho c}{4k}\right)^2 \right\}.$$

Thus the temperature at depth x, counted from the plane $x = 0$, gradually commences to rise and continues to do so for a time

$t = x^2\rho c/2k$, after which it steadily falls. The times at which the increase and decrease are fastest are respectively the smaller and the greater root of the quadratic equation

$$\tfrac{3}{4}t^2 - 3\frac{x^2\rho c}{4k}t + \left(\frac{x^2\rho c}{4k}\right)^2 = 0 ; \quad \ldots \ldots \quad (2)$$

and are approximately $\cdot 0917\dfrac{x^2\rho c}{k},$ . . . . (3)

and $\cdot 908\dfrac{x^2\rho c}{4k}.$ . . . . (4)

Supposing it were possible suddenly to supply a quantity of heat to the surface of the liquid in the tub and to ensure that no commensurable quantity was subsequently gained or lost, an observation of the time at which the temperature at a given depth was rising fastest, or was stationary, would enable k to be determined at once.

In the actual case the problem is more complex as $f(\chi)$, though diminishing rapidly as χ increases, is different from zero ; the principle however is practically unchanged. By differentiation we obtain from (1)

$$2\sqrt{\frac{\pi k}{\rho c}}\frac{d^2v}{dt^2} = \int_0^t f(\chi)\frac{d^2}{dt^2}\left\{\frac{e^{-x^2\rho c/4k(t-\chi)}}{\sqrt{(t-\chi)}}\right\}d\chi$$

$$+ \text{terms at the limits.} \quad \ldots \ldots \quad (5)$$

Now when t is moderately large the terms at the limits may be neglected. This follows from a consideration either of the mathematical form or the physical meaning of those terms. They are proportional, one to the temperature instantaneously produced at the depth x at the time t by the heat at that instant passing into the liquid from the dish, and the other to the rate of change of this instantaneous effect. Now even when heat is being very rapidly communicated to the liquid, as at the commencement of the experiment, the rise in temperature due to conduction at a moderate depth is for a minute or two insignificant. Thus when the heat is being communicated very slowly, as is the case at the time at which we shall employ (5), the terms at the limits are for all practical purposes negligible.

When the temperature of the liquid at depth x is rising most rapidly, dv/dt is a maximum, and so $d^2v/dt^2 = 0$. From the above reasoning it follows that the time in question must satisfy the equation—

$$0 = \int_0^t f(\chi)(t-\chi)^{-9/2}e^{-x^2\rho c/4k(t-\chi)}\left\{\tfrac{3}{4}(t-\chi)^2 - 3\frac{x^2\rho c}{4k}(t-\chi)\right.$$

$$\left. + \left(\frac{x^2\rho c}{4k}\right)^2\right\}d\chi. \quad \ldots \ldots \ldots \quad (6)$$

This equation cannot be exactly solved, but an approximate solution of sufficient accuracy can be obtained. This gives t as a function of x, ρ, c, and k; but t is determined from the galvanometer readings, and x, ρ, and c can be otherwise determined, thus k is at once obtainable.

With the smaller apparatus, when the dish remained unemptied, the value of t, when water was in the tub, exceeded ten minutes, and for nearly all other liquids it is greater. The integral can be replaced by the summation—

$$\Sigma \, (t-\chi)^{-9/2} \, e^{-x^2\rho c/4k(t-\chi)} \left\{ \tfrac{3}{4}(t-\chi)^2 - 3\frac{x^2\rho c}{4k} (t-\chi) + \left(\frac{x^2\rho c}{4k}\right)^2 \right\} \mathrm{Q}\tau \; = \; 0,$$

$$\ldots \ldots (7)$$

where $\mathrm{Q}\tau$ is proportional to the heat transmitted to the liquid during the interval τ, and $t - \chi$ is the time between the middle of this interval and the epoch of swiftest rise of temperature. It is not necessary to take τ the same throughout; thus at the beginning of the experiment when $f(t)$ varies rapidly, τ must be taken smaller than subsequently. The terms in the summation answering to the last few minutes of the experiment are always very small.

When the water was siphoned from the dish, any gain or loss of heat through the dish subsequent to the operation was very small compared to that given up to the liquid previously. Thus no serious error will be introduced by supposing $f(t) = 0$ after the siphoning.

It will be observed that what the galvanometer readings give is the time when the platinum wire is heating fastest, while the equation gives this epoch for the liquid at the same depth as the wire. Since the temperatures of the media are changing very slowly, it is scarcely conceivable that they could differ by a finite quantity, or that their rates of change should not be practically alike. The assumption made in the present method is of a totally different order from that made by previous observers dealing with thin layers of liquid. Their assumptions would be equalled only by supposing the dish and the liquid touching it to be always identical in temperature.

Theoretically the absolute quantity of heat initially given to the dish is of no importance, except in so far as it modifies the rate at which heat is subsequently communicated to the liquid. Experimentally it was found that both the quantity and the temperature of the water poured into the dish could be varied to a considerable extent without sensibly altering the epoch of quickest rise of temperature. When the water was siphoned the initial quantity of heat was of still less importance. With most liquids, however, the water was heated to a fixed temperature, viz., 75° C., and a measured

quantity was poured into the dish. With bisulphide of carbon of course a much lower temperature was employed, but even then its heated top layer evaporated so fast as to affect the contact of the liquid with the dish. For these reasons in all experiments on the bisulphide the water was siphoned out of the dish.

In the earlier part of each experiment the heat was of course concentrated chiefly in the upper layers. Still as about an inch intervened between the dish and the platinum, the variation of temperature in by far the greater portion of the liquid layer was comparatively small. Thus the error due to treating the conductivity as independent of the temperature cannot be great.

There are two possible disturbing agencies which require comment. Any difference of temperature between the two copper-platinum junctions in the liquid would produce a thermoelectric current. Care was taken, however, that the junctions should be as nearly as possible in the same horizontal plane. At the depth in question the greatest possible difference between the temperatures at two points differing a few millimetres in depth could not exceed a small fraction of one degree. Thus the thermoelectric current, if existing, must have been very small, and necessarily its variation, from which alone any error could arise, must have been very trifling. Further, the neutral point of copper and platinum is only about 70°, so they would under the circumstances form a very weak couple.

An attempt was in fact made to employ a thermoelectric couple of iron and lead, whose neutral point is over 350°, one junction being in the liquid and the other maintained at a constant temperature. This, however, failed completely, owing to want of sensitiveness. Thus there are various à priori grounds for neglecting the thermoelectric effect in the actual experiment. This view was further justified by actual trial, first by finding the deflection that followed when one of the junctions was suddenly heated to a considerable temperature, second by cutting out the battery during the usual experiment, and observing whether shunting the platinum wire affected the galvanometer. Finally, in the various experiments on any one liquid the battery current traversed the platinum wire sometimes in one direction sometimes in the other; and thus any possible thermoelectric effect must have tended sometimes to increase and sometimes to diminish the rate of variation of the galvanometer reading. The small variation in the observed times of most rapid variation is thus sufficient proof of the small disturbing action of the thermoelectric effect, and the variation in the direction of the battery current would further tend to eliminate any such small effect if existent.

Another disturbing cause existed in the case of the sulphuric acid solutions. These conducted electrolytically, and also attacked the copper wires. By covering these wires with shellac varnish this was

prevented. With the weaker solutions a slight coating was sufficient, but with the stronger it had to be frequently renewed.

In the cooling experiments, if we suppose the mass of water in the dish to remain constant, the rate at which the thermometer falls at any instant is directly proportional to the rate at which heat is leaving the dish. Of this heat some passes into the material below the dish, and some is lost by radiation from the water and the sides of the dish. The material on which the cooling was slowest was packing, and the heat given up to it seemed very small. Of this by far the greater part occurred in the first minute, and this is precisely the time when a small error in calculating the heat given to the liquid in the experiments on conductivity is of least importance. Thus in default of more accurate knowledge, the loss of heat by the dish when on packing was taken as representing the loss by radiation when on a liquid. Since the absolute amount of heat given to the liquid is not required, but only the ratio of the quantities given up for each minute or half minute of the experiment, absolutely no error would be introduced by neglecting the conductivity of the packing, provided the heat passing into the packing followed the same law as that passing into the liquid.

The liquids whose conductivities were determined are water, sulphuric acid solutions of various strengths, bisulphide of carbon, one solution of methylated spirit, paraffin, and turpentine oils. For water, methylated spirit, and paraffin two series of observations were made, the water being siphoned from the dish in one case, and left in the other. In the case of turpentine no observations were made with the water siphoned. For the sulphuric acid solutions and the bisulphide of carbon the siphon was always used. It was found that the conductivity and the rate of cooling of the dish were nearly independent of the strength of the sulphuric acid solution, and differed little from the corresponding quantities for water. The law of cooling on bisulphide of carbon also closely resembled that on water. The relative conductivities of these liquids would thus in all probability be most correctly obtained by referring them to the value obtained for water by the method employing the siphon. The following table gives the quantity of heat given up to the liquids in consecutive minutes or half minutes of the experiment, so far as is required in calculating the conductivity. The unit employed is arbitrary, but is the same throughout:—

Mr. C. Chree.

Table I.

Time in minutes.	Water.	Methylated spirit.	Paraffin oil.	Turpentine oil.	Bisulphide of carbon.	Sulphuric acid solution, density 1·2.
0— ½	606	416	133	170	510	587
½— 1	386	327	136	137	270	422
1— 1½	241	157	110	93	80	250
1½— 2	185	119	79	67		
2— 3	255	170	100 ·7	82		
3— 4	181	100	72·1	43		
4— 5	138	79	50·2	32		
5— 6	112	67	34·4	22·6		
6— 7	92	53	28·4	16·6		
7— 8	74	38	17·0	6·3		
8— 9	64	26	13·8	·7		
9—10	58	21	11·5			
10—11	..	18	7·3			
11—12	..	16	6·7			
12—13	..	15	..			
13—14	..	..	..			

The times required for the dish to cool from 75° to 30° on water and on the sulphuric acid solution were almost identical. Excluding the first half minute, the cooling on the solution was slightly but decidedly faster for the first half of the period. In the bisulphide of carbon the experiment was made at a much lower temperature, which accounts for the comparatively small quantity of heat given to the liquid in the third half minute. In fact, considering the small temperature excess the rate at which the dish lost heat on the bisulphide was initially extremely rapid. On turpentine for the first few minutes the loss of heat was much faster than on packing; the rates then began to approach, and after the first nine minutes could not with certainty be said to differ. This coincidence lasted for the next five minutes or more, during which the observations were continued. Excluding the first minute, the cooling on turpentine was very decidedly slower than on any other liquid. This is due to the low conducting power and small specific heat of the liquid, in virtue of which the top layer soon became a sort of barrier to the penetration of the heat.

The following table gives the density ρ, and specific heat, c, of the liquids, and the time, t, in minutes, after the heating commenced before the temperature of the platinum wire was rising fastest :—

Table II.

Liquid.	ρ.	c.	ρc.	t. Water siphoned.	t. Water not siphoned.
Water	1·0	1·0	1·0	9·0	10·7
Sulphuric acid solution	1·054	0·935	0·985	8·75	
Do.	1·1	0·877	0·965	8·5	
Do.	1·14	0·843	0·961	8·5	
Do.	1·18	0·802	0·946	8·25	
Bisulphide of carbon......	1·276	0·247	0·315	6·65	
Methylated spirit	0·849	0·675	0·573	10·75	13·2
Paraffin oil	0·803	0·50	0·401	10·25	12·25
Turpentine oil............	0·875	0·43	0·376	..	14·6

In each case the density was taken with a hydrometer at or near the temperature of the experiment, and the specific heats are taken directly from 'Watts' Dictionary of Chemistry,' or obtained by inter-polation from tables given there. In the case of the last three liquids the specific heat was also obtained experimentally, as these liquids vary somewhat in composition. The results so obtained were some-what rough, but were sufficiently good to act as a check on the values taken. Small errors in the specific heat are not of much im-portance, as the probable error in the experiments on the conductivity amounts to at least several per cent. of the numbers taken. Further, the mean temperature to which the conductivity should be referred is also a somewhat doubtful matter.

The introduction of small impurities in the liquids did not appre-ciably alter t. For instance, small quantities of salt were put into the methylated spirit, and small quantities of the latter into water without producing any apparent effect. It would thus appear that the absolute purity of the liquids used is not of much consequence. Care was, however, taken to keep them as pure as possible, the bisul-phide of carbon in particular being redistilled before use.

In every case the value t is the mean of a good many experiments, and, as a rule, the individual experiments agreed well together. Thus, when the siphon was not used the values obtained for t varied from 10·3 to 11 for water, from 12·75 to 14 for methylated spirit, and from 14·25 to 15 for turpentine. When the siphon was used the extreme differences in the numbers obtained for t were about as large as in the other method, and thus the agreement between the experiments was really not quite so good. This was only to be expected, as there was necessarily some slight variation in the time taken to siphon and in the result of the operation.

In obtaining a mean value for t the following method was adopted :

—A table was formed giving the increase in the galvanometer readings for each minute of each experiment. If there had been much variation in the sensitiveness of the galvanometer, the numbers obtained from each experiment were multiplied by a number varying inversely as the total increase in the readings during the fifteen minutes of that experiment subsequent to the application of the heat. The numbers for each minute were then added together, and the sum gave the mean rate of heating for the minute in question. From these rates the time of fastest heating could be easily calculated, or could be obtained graphically by constructing the curve whose abscissæ were the times elapsed since the heating, and whose ordinates were proportional to the rates of heating.

It will be best to consider first the experiments in which the water was siphoned from the dish, as the arithmetic required to obtain the conductivity from the equation (7) is then comparatively simple. It is assumed that the heat passed into the liquid for the first three half-minutes according to the law given in Table I, and that subsequently no heat at all was either given or lost through the dish. As it took some time to perform the siphoning, and there were no doubt slight variations in the small quantity of water left in the dish, the above is only approximately true, but the multiplication of the observations would tend to eliminate the errors.

If $x^2 \rho c/4k$ be denoted by X, then from (7) and Table I, since $t = 9$, we have for water—

$$606(8 \cdot 75)^{-9/2} e^{-X/8 \cdot 75}\{X^2 - \tfrac{3}{4}X(8 \cdot 75) + \tfrac{3}{4}(8 \cdot 75)^2\}$$
$$+386(8 \cdot 25)^{-9/2} e^{-X/8 \cdot 25}\{X^2 - \tfrac{3}{4}X(8 \cdot 25) + \tfrac{3}{4}(8 \cdot 25)^2\}$$
$$+241(7 \cdot 75)^{-9/2} e^{-X/7 \cdot 75}\{X^2 - \tfrac{3}{4}X(7 \cdot 75) + \tfrac{3}{4}(7 \cdot 75)^2\} = 0.$$

From this equation X must be obtained by trial. If U stand for the left-hand side of the equation, it will be found that the corresponding values of X and U are as follows:—

X.	U.
22	$-88 \cdot 6 \times 10^{-3}$
22·7	$- 8 \cdot 5 \times 10^{-3}$
22·8	$+ 1 \times 10^{-3}$

The value of U is best found by considering the logarithm of the several lines in succession. The following are the values of these lines:—

X.	First line of U.	Second line.	Third line.
22	$-101 \cdot 8 \times 10^{-3}$	$-12 \cdot 7 \times 10^{-3}$	$+25 \cdot 9 \times 10^{-3}$
22·7	$- 60 \cdot 5 \times 10^{-3}$	$+10 \cdot 21 \times 10^{-3}$	$+41 \cdot 77 \times 10^{-3}$
22·8	$- 55 \cdot 1 \times 10^{-3}$	$+12 \cdot 05 \times 10^{-3}$	$+44 \cdot 05 \times 10^{-3}$

From the values of U a very close approximation to X may be obtained by Maclaurin's theorem, which gives X = 22·79. This is a much closer degree of approximation than is at all necessary, considering the possible size of the experimental errors.

Since $x = $ depth of wire $ = 2$·61 cm.,

we get finally, the units being centimetre and minute,

$$k = \frac{6 \cdot 8121}{91 \cdot 16}$$

$$= 0 \cdot 0747.$$

This corresponds to a temperature of about 18° C.

The law of cooling of the dish on sulphuric acid solutions so nearly resembled that on water, that it will be sufficient to take the results given in Table I for a solution of density 1·2, and combine them with the corresponding results for water in a ratio proportionate to the strength of the intermediate solution. We thus obtain as proportional to the heat given to the liquid in the first three half-minutes the following values :—

Density of solution.	First half-minute.	Second half-minute.	Third half-minute.
1·054	601	396	243
1·11	596	404	245
1·14	593	411	247
1·18	589	418	249

Employing U and X in the same sense as for water, it will be found, precisely as in the previous case, that the following results are true for the various solutions. The last column gives the temperature to which the conductivity belongs.

Table III.

Density of solution.	X.	U.	Mean value of U.	k.	Temperature, centigrade.
1·054	22·0 22·1 22·2	$-10^{-3} \times 12$·4 $+10^{-3} \times 0$·64 $+10^{-3} \times 13$·58	22·095	0·0759	$20\frac{1}{2}$
1·10 ..	21·4 21·6	$-10^{-3} \times 2$ $+10^{-3} \times 28$·4	21·413	0·0767	$20\frac{1}{4}$
1·14 ..	21·4 21·6	$-10^{-3} \times 1$ $+10^{-3} \times 29$·6	21·407	0·0765	$19\frac{3}{4}$
1·18 ..	20·6 20·8	$-10^{-3} \times 17$ $+10^{-3} \times 16$	20·703	0·0778	21

Neither the method nor the theory is so extremely accurate that any value can be attached to the third significant figure in the value for k in assigning absolute values for the conductivity. In assigning relative values, the third figure would have some weight in liquids in which the heat was applied so similarly as in the case of water and the above solutions. Since, however, the temperatures of the experiments were not identical, and the conductivity unquestionably increases with the temperature, it would probably be unsafe to deduce from the above numbers any more precise conclusion than that the presence of a very considerable quantity of sulphuric acid produces an extremely small change in the conductivity for heat of water.

It might also be considered almost certain that the time at which the temperature was rising fastest diminished as the density of the solution increased. This signifies that the velocity with which heat-waves travel, or the *temperature conductivity* of Weber, is greater the stronger the solution. The liquid in the tub in these experiments was in general stirred up fifteen minutes after the heat had been applied. It is pretty obvious that the ratio of the galvanometer reading after the stirring to that before should diminish as the temperature conductivity increases. The ratios so obtained for water and the above solutions were in order, 1·36, 1·34, 1·32, 1·28 and 1·23.

No very great accuracy can be claimed for these numbers as the determination was somewhat rough, but as independent evidence of the truth of the above statement as to the temperature conductivity they are of considerable weight.

For bisulphide of carbon, methylated spirit, and paraffin oil, the values from Tables I and II substituted in equation (7) lead to the following results :—

Table IV.

Liquid.	X.	U.	Mean value of X.	k.	Mean temperature.
Bisulphide of carbon.	16·5 16·6 16·7	$-10^{-4} \times 36·16$ $-10^{-4} \times 15$ $+10^{-4} \times 9·26$	16·66	0·0322	$15\frac{1}{2}°$
Methylated spirit....	27·5 27·6	$-10^{-4} \times 26·5$ $-10^{-4} \times 13·8$	27·566	0·0354	$19\frac{1}{2}$
Paraffin oil	25·8 26·0	$-10^{-4} \times 18·66$ $+10^{-4} \times 26·66$	25·88	0·0264	19

There still remain to be considered the experiments in which the water was not siphoned from the dish. As an example of the ap-

plication of (7), it may be as well to give its form for one of the liquids. The following equation is for the methylated spirit, X being $= x^2\rho c/4k$ as previously.

$$416(12{\cdot}95)^{-9/2}\, e^{-X/12{\cdot}95}\{X^2 - 3X(12{\cdot}95) + \tfrac{3}{4}(12{\cdot}95)^2\}$$
$$+\, 327(12{\cdot}45)^{-9/2}\, e^{-X/12{\cdot}45}\{X^2 - 3X(12{\cdot}45) + \tfrac{3}{4}(12{\cdot}45)^2\}$$
$$+\, 157(11{\cdot}95)^{-9/2}\, e^{-X/11{\cdot}95}\{X^2 - 3X(11{\cdot}95) + \tfrac{3}{4}(11{\cdot}95)^2\}$$
$$+\, 119(11{\cdot}45)^{-9/2}\, e^{-X/11{\cdot}45}\{X^2 - 3X(11{\cdot}45) + \tfrac{3}{4}(11{\cdot}45)^2\}$$
$$+\, 170(10{\cdot}7)^{-9/2}\, e^{-X/10{\cdot}7}\{X^2 - 3X(10{\cdot}7) + \tfrac{3}{4}(10{\cdot}7)^2\}$$
$$+\, 100(9{\cdot}7)^{-9/2}\, e^{-X/9{\cdot}7}\{X^2 - 3X(9{\cdot}7) + \tfrac{3}{4}(9{\cdot}7)^2\}$$
$$+\;\; 79(8{\cdot}7)^{-9/2}\, e^{-X/8{\cdot}7}\{X^2 - 3X(8{\cdot}7) + \tfrac{3}{4}(8{\cdot}7)^2\}$$
$$+\;\; 67(7{\cdot}7)^{-9/2}\, e^{-X/7{\cdot}7}\{X^2 - 3X(7{\cdot}7) + \tfrac{3}{4}(7{\cdot}7)^2\}$$
$$+\;\; 53(6{\cdot}7)^{-9/2}\, e^{-X/6{\cdot}7}\{X^2 - 3X(6{\cdot}7) + \tfrac{3}{4}(6{\cdot}7)^2\}$$
$$+\;\; 38(5{\cdot}7)^{-9/2}\, e^{-X/5{\cdot}7}\{X^2 - 3X(5{\cdot}7) + \tfrac{3}{4}(5{\cdot}7)^2\}$$
$$+\;\; 26(4{\cdot}7)^{-9/2}\, e^{-X/4{\cdot}7}\{X^2 - 3X(4{\cdot}7) + \tfrac{3}{4}(4{\cdot}7)^2\}$$
$$+\;\; 21(3{\cdot}7)^{-9/2}\, e^{-X/3{\cdot}7}\{X^2 - 3X(3{\cdot}7) + \tfrac{3}{4}(3{\cdot}7)^2\}$$
$$+\;\; 18(2{\cdot}7)^{-9/2}\, e^{-X/2{\cdot}7}\{X^2 - 3X(2{\cdot}7) + \tfrac{3}{4}(2{\cdot}7)^2\}$$
$$+\;\; 16(1{\cdot}7)^{-9/2}\, e^{-X/1{\cdot}7}\{X^2 - 3X(1{\cdot}7) + \tfrac{3}{4}(1{\cdot}7)^2\}$$
$$+\;\; 15({\cdot}7)^{-9/2}\, e^{-X/{\cdot}7}\{X^2 - 3X({\cdot}7) + \tfrac{3}{4}({\cdot}7)^2\} \;=\; 0.$$

The solution must of course be obtained by trial, but it is comparatively easy to form a pretty accurate idea of its value from considering the value of the coefficients in square brackets. Further, when a solution has been obtained for one equation, its magnitude enables an idea of the magnitude of the solutions of the other similar equations to be readily obtained. The necessary arithmetic is best performed by finding the value of each line of the left-hand side separately by means of logarithms. The first four or five lines will in each case be negative, and the rest positive. The last two lines at least will be found extremely small. The following table, in which the letters have their previous significations, gives the results obtained:—

Mr. C. Chree.

Table V.

Liquid.	X.	U.	Mean value of X.	k.	Tempera-ture.
Water	21 22	$+10^{-3} \times \ \ 13 \cdot 4$ $+10^{-3} \times 149 \cdot 1$	$20 \cdot 9$	$0 \cdot 0815$	$19\frac{1}{2}°$
Methylated spirit....	27 28	$-10^{-3} \times \ \ 57 \cdot 45$ $-10^{-3} \times \ \ \ 8 \cdot 3$	$28 \cdot 17$	$0 \cdot 0346$	18
Paraffin oil	25 $25 \cdot 1$ 26	$-10^{-3} \times \ \ \ \ 1 \cdot 04$ $+10^{-3} \times \ \ \ \ 2 \cdot 52$ $+10^{-3} \times \ \ 30 \cdot 28$	$25 \cdot 029$	$0 \cdot 0273$	20
Turpentine oil	33 34	$-10^{-3} \times \ \ 12 \cdot 36$ $+10^{-3} \times \ \ \ \ 1 \cdot 97$	$33 \cdot 86$	$0 \cdot 0189$	18

As an example of the arithmetical results and the value of successive lines of U, it will be as well to take as an example the results in the case of methylated spirit, which are embodied in the following table :—

Table VI.

Number of line.	X = 27.	X = 28.
1	$-10^{-6} \times 99223$	$-10^{-6} \times 84211$
2	71974	59235
3	30661	24104
4	19382	14098
5	16479	8377
6	$+10^{-6} \times \ \ 3110$	$+10^{-6} \times \ \ 8043$
7	17016	20528
8	30864	32878
9	39736	39657
10	38564	36556
11	28695	25796
12	17346	14560
13	4827	3636
14	109	67
15	$10^{-13} \times \ \ \ \ 1$	$10^{-13} \times \ \ \ \ 2$

Taking into consideration the nature of the investigation, the agreement between the results obtained by the two methods seems on the whole satisfactory. In the case of methylated spirit and paraffin oil, the agreement could hardly be closer, and the fact that it is so good must indeed in considerable measure be a pure matter of

chance. In the case of water, there is a decided though not very serious discrepancy. The difference in the mean temperature of the two experiments could account for only a small part of this. The experiments on water were the earliest in which the siphon was used, and the operation took slightly longer and its results were not quite so uniform as in later experiments. Further, when no siphoning took place, the heat passing into the liquid at the end of the experiment was much larger in the case of water than for the other liquids, and the terms at the limits in (5) would thus be of slightly greater importance for water than for the others. Also an error of given amount in the experimental determination of the time of most rapid heating would produce the greater effect the shorter the time, and would thus modify the results for water more than for any other of the liquids, except bisulphide of carbon. Thus it was only to be expected that the greatest discrepancy between the results of the two methods should occur in water.

With the larger apparatus results were obtained for water and methylated spirit, of the same constitution as in the experiments already described, which, though not pretending to great accuracy, may be of interest as independent evidence of the correctness in the main of the theory. For the intervals in minutes that elapsed after the application of the heat before the temperature of the wire was rising fastest, the mean of several experiments gave $52\frac{1}{4}$ for water, $67\frac{1}{2}$ for the spirit. The water was left undisturbed in the dish, and no accurate observations of the rate of cooling were made. It was noticed, however, that the dish parted rapidly with its heat, and being only slightly deeper than the dish in the small apparatus, it is pretty clear that by far the greater part of the heat was given to the liquid in the tub in the first few minutes. Thus the experiment would be pretty much akin to the case when the water was siphoned in the smaller apparatus. Though ignorant of the law of cooling, we can thus obtain an inferior limit to the conductivity, of a moderately close kind, by supposing the heat to have been instantaneously communicated. This gives from expression (3), viz., $k = 0\cdot0917x^2\rho c/t$, for water $k = 0\cdot0730$, and for methylated spirit $k = 0\cdot0324$, corresponding to temperatures of about $18°$ C. These results as being essentially inferior limits, agree fairly with those of the smaller apparatus.

On the whole, the results of this series of experiments resemble those obtained by Herr Weber.* The values obtained for the conductivity of water agree fairly well with his. The smaller value obtained by Weber for bisulphide of carbon, viz., $0\cdot0250$, would be partly accounted for by the very considerably lower temperature of his experiment. As this liquid boils at a very low temperature, the

* 'Wiedemann, Annalen,' vol. 10, pp. 103, 304, 472; see specially table on p. 314.

rate of variation with the temperature of its thermal conductivity is very probably much above the average.

To reduce the results of the present paper to the C.G.S. system of units, it is only necessary to divide them by 60.

"On Rabies." By G. F. DOWDESWELL, M.A., F.L.S., F.C.S. Communicated by Professor VICTOR HORSLEY, F.R.S. Received May 9,—Read June 16, 1887.

[PLATE 1.]

CONTENTS.

Numerous as are the communications upon the subject of rabies, the paucity of experimental investigation is remarkable; the disease has remained for upwards of 2000 years, since the first recorded mention of it by Aristotle, exceedingly obscure in many essential points. The unparalleled and variable length of its incubation period has offered the greatest obstacle to systematic examination; in the words of John Hunter in the last century, "It has defied alike scientific investigation as to its intimate nature, and all remedial measures for its successful treatment."

Lately, however, the results announced to have been attained by M. Pasteur, have promised to remove these obstacles, and encouraged research by new methods and with fresh views.

This investigation was commenced early in 1885, during the prevalence of rabies in and around London. Two well-marked cases in dogs were obtained, and inoculations with their saliva, taken both during life and shortly after death, were made into the subcutaneous tissue of other animals, but failed to produce infection.

At that time I was not sufficiently conversant with the results of M. Pasteur's investigations to place reliance upon his methods of intracranial inoculation with the cerebro-spinal substance of a rabid animal, and I must admit that his statements seemed to me to be im-